ERC Advanced Grant 2022
Research proposal [Part B1]

A Hope for Particle Physics

VIRTUAL

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- Duration: 60 months

The physics of fundamental interactions is going through a concerning, prolonged period of stagnation. The incredible success of the standard model of particle physics and the lack of new experimental data have frustrated our hopes in the future. On top of that, the scientific community shattered into a large number of isolated groups. Many mainstreams have consolidated, leaving not much room for the advancement of bright, original proposals. In frontier domains, like quantum gravity, most mainstreams have disavowed the inheritance of the glowing past and embarked on uncertain routes (string theory, loop quantum gravity and many others). It is time to make room for approaches that are really out of the box and can truly trigger a renaissance of particle physics. Yet, they can only be believable if they are solidly rooted in the successes of the past. This ERC project pursues a research line that does stem from the achievements of the past, but is radically new and has the potential to take us out of this dark period. It is based on the notion of purely virtual particle, which upgrades in a crucial way our understanding of fundamental interactions through quantum field theory. One of its key predictions in primordial cosmology could be confirmed experimentally within a decade. Nevertheless, the scientific community cannot afford another decade like the past ones, so it is imperative to act now. The new idea opens the door to unthinkable scenarios and has a huge amount of ramifications and applications to all areas of fundamental physics, with the potential to build bridges between quantum gravity, primordial cosmology and the phenomenology of particle physics beyond the standard model. More key predictions are expected to follow, together with crucial ideas for future colliders. Hopefully, they will trigger the breakthroughs that we need to make a U turn, activate a virtuous circle, reunite the scientific community and lead to the renaissance of particle physics.

The purpose of the project is to build bridges between different domains of high-energy physics, specifically: the phenomenology of particle physics beyond the standard model, quantum gravity and primordial cosmology. It also aims at disseminating knowledge of fundamental science to the wider public. The panel "PE2 – Fundamental constituents of matter" has experts in these fields who can fairly evaluate the project

Section a: Extended Synopsis of the scientific proposal (max. 5 pages, references do not count towards the page limits)

Particle physics is experiencing a prolonged period of stagnation, with no way out in sight. Our hopes in the future of fundamental physics have been frustrated by the lack of experimental data, the difficulties to make new experiments, the huge costs and amount of time (around half a century) needed to plan and build new accelerators. It is time to make room for approaches that are really out of the box. Yet, they can only be believable if they are solidly rooted in the successes of the past. The aim of the present project is to pursue a radically new, bold research line, which does stem from the successes of the past and has so many ramifications and applications to potentially trigger the renaissance of fundamental physics.

An unexpected, concerning consequence of the present impasse is the fragmentation of the scientific community of theoretical physicists into a plethora of basically isolated subcommunities, which do not even talk to one another anymore, and go on investigating hyper pursued research lines, looping repetitively. To give you a vivid image of what I mean, I can list the numerous proposals for quantum gravity that are pursued in this very moment, despite the lack of experimental data backing any of them. They go under the names of: string theory, loop quantum gravity, the AdS/CFT correspondence, holography, asymptotic safety, nonlocal quantum gravity, causal dynamic triangulations, causal fermion systems, causal set theory, canonical quantum gravity, supergravity, twistor theory, non commutative quantum gravity, Regge calculus, double copy theory, preons... And so on and so forth. A large number of conferences are held every year, in each subdomain, and a huge number of papers are published, as well as volumes and special issues. Nevertheless, no tangible progress that might change the direction of fundamental physics has been made in decades. This frustrating situation has lead to widespread sentiments of hopelessness and depression. The critical spirit has almost evaporated completely. Cross checking, questioning or hard challenging have basically disappeared. The situation is so serious that nowadays new, ambitious ideas are viewed suspiciously, when not ignored thoroughly. Instead of welcoming stimulating new challenges, people prefer to stick to much of the same and plunge into revivals of old things. Scientists are stressed everyday with multiple distractions and cannot find the rosebud. This is why I am urging the European Union to fund ERC grant projects that have the chance to revert the situation, with no further ado.

A cause of the present stalemate is the incredible success of the standard model of particle physics, which explains three of the four fundamental interactions of nature. Success after success, confirmation after confirmation, the standard model has defied all attempts to extend it, or surpass it. The discrepancies between the predictions of the theory and the experimental data are so weak that we can consider them as basically non significant. However, the success of the standard model, which elevated European particle physics to the top of the world, belongs to the far past. The discovery of the Higgs boson in 2012 was predicted almost 50 years earlier by us theorists. Since then, no substantial progress in particle physics has been made. The more recent speculations that have been put forward by theoretical physicists around the world have been repeatedly refuted by nature. For example, the idea of grand unification of interactions may sound very appealing to us. Unfortunately, nature does not seem to like it, to the extent that it rejected it almost as soon as it was proposed. Supersymmetry, which has been the main paradigm for decades in particle physics, has been rejected by nature ten years ago, thus frustrating all efforts (and money) spent to fund its theoretical pursue. String theory, loop quantum gravity and the other approaches to quantum gravity mentioned above do not even have the ambition to make testable predictions.

Too much success before, too much failure later. Today, it is extremely hard to get due attention when you do not repeat what everybody says. In such a situation, theoretical physics and the investigation of the fundamental interactions of nature have become a sort of “talk the talk..., talk the talk”.

The PI has directed his entire research activity along a different path and claims that he can offer the solution to the impasse. Not another approach of the many, but the only hope we have to get out of the abyss.

What have the proposals listed above in common with one another? Certainly, they are not solid. Why should the universe be a hologram? What indications have we ever had of anything like this in the past? And what about strings? Have there ever been indications of strings in quantum electrodynamics, general relativity, chromodynamics, the weak interactions, i.e., in the advances that helped us build the standard model? None whatsoever. What about the loops on which loop quantum gravity relies? And why should we believe in the dogma of asymptotic safety? Don’t these approaches sound a bit too far from reality? How many chances do we have to predict nature by making proposals on the basis of human aesthetical or social tastes? Why should nature, at the infinitesimally small scales of magnitude (a billionth of a billionth of an atom), conform to our own tastes and wishes? Why should the intuitions suggested by the world up here be adequate to guess the unknown down there? Isn’t it time to change direction, or close the field forever?

If you think it is time to close the field forever (which is a legitimate opinion, I have to say), then you can stop reading this proposal right now. But if you are open to give particle physics a last chance, there is no time to waste. And I am the only candidate who can make the hope come true.

The PI's attitude is and has always been critic towards the mainstreams. If it were possible to rely on mainstreams to solve a problem like this one, we would not be in this situation in the first place. No: the solution must be somewhere else. And we cannot venture into other gratuitous endeavours like the one mentioned above, since there are no experimental data that may back them.

The only possibility is that the answer has always been in front of our eyes. And for the reasons listed above, we could just not see it. We can find it if we stick to what has worked so well so far, and concentrate on that, search for the key hints there, instead of wondering forever in our own fantasies.

What if quantum gravity were just a step away from the standard model? A fairly guessable missing piece of the puzzle?

Here is a picture of the "puzzle of fundamental physics", as we can summarize it right now.

<table>
<tr><th colspan="2">Classical physics</th><th colspan="2">Quantum physics</th></tr>
<tr><td rowspan="3">Classical mechanics</td><td>Electrodynamics</td><td rowspan="3">Quantum mechanics</td><td>Quantum electrodynamics</td></tr>
<tr><td>Special relativity</td><td>Standard Model</td></tr>
<tr><td>General relativity</td><td>Quantum gravity</td></tr>
</table>

Note that electrodynamics has been an extremely precious guide for every further progress. After all, it is the interaction of nature that provides us with... *light*. Both at our scales of magnitude and the smaller ones, it has been the source of a huge amount of data. The standard model of particle physics was basically built on it. The intermediate bosons W and Z were introduced to mimick the diagrammatics of electrodynamics as close as possible. With one important novelty: the Higgs boson, which was needed to give mass to the "photons" of the weak interactions. Similarly, quantum chromodynamics is a set of "self-interacting photons". With another important novelty with respect to electrodynamics: asymptotic freedom.

Unequivocally, electrodynamics has been the guide and the paradigm. At the quantum level, electrodynamics means interactions between light and electrons. Theoretically speaking, this means Feynman diagrams. The two top right cells of the table, quantum electrodynamics and the standard model of particle physics, are indeed built on Feynman diagrams. The bottom right cell, quantum gravity, is the one we have to fill, the missing piece of the puzzle. Is it wise to abandon the path that has worked so well so far, the route traced by Feynman diagrams, to embark on uncertain approaches based on our personal or social inclinations?

If the book of nature is written in mathematical language, quantum electrodynamics and the standard model of particle physics have taught us that the language of particle physics is diagrammatic. Which means: scattering processes, particle collisions, resonance peaks, i.e., the ingredients that triggered a one-century-long history of successes. Not strings, not the loops of loop quantum gravity, holograms, triangulations, causal sets or preons, but just Feynman diagrams, like the ones that made us discover the Higgs boson at CERN in 2012, the top quark at Fermilab in 1995, the W and Z bosons at CERN in 1982-3, and all the other quarks before that. If there is any hope to trigger a renaissance of fundamental physics and build the future from there, we should examine carefully what has worked so far. Since experimental data are missing, since we have no fresh hint, that is our only chance.

And there you find it indeed. The last piece of the puzzle, the closure of the circle, the possibility that was missed: **the purely virtual particle**.

A normal elementary particle, such as the electron or the photon, can be real or virtual, depending on whether it is observed or not. A particle that is always real cannot exist. What about a particle that is always virtual and can never become real? Such an entity must mediate interactions among other particles, but remain invisible to our detectors. Said in different words, it is a particle that cannot be conceived by "quantizing" a classical system, because it is of a purely quantum nature.

It turns out that such a particle is mathematically allowed and can be identified by means of a new diagrammatics [1]. Yet, the new diagrammatics is not a shot in the dark, because it is rooted in the usual one. Actually, it can be obtained from the usual one by means of surgical operations that remove degrees of freedom from crucial places. And such operations are simple enough that they can be implemented in existing software like FeynCalc, FormCalc, Looptools, Package-X, etc., to make physical predictions in model building of new physics beyond the standard model.

Just one (very heavy) purely virtual particle, of spin 2, is enough to fill the bottom-right cell of the puzzle, i.e., make sense of quantum gravity [2]. But the crucial thing is that the theory emerging from the new concept is predictive, testable and falsifiable. Not only: it can be tested within our lifetime [4]! This is the most important piece of information backing this research proposal. Pay attention to the following crucial number of primordial cosmology: r. It will be measured in the incoming years, in experiments like LiteBird and Bicep/Keck. Its value will probably make big news no matter what. It is called "tensor-to-scalar ratio" and expresses the ratio between the two basic power spectra of the primordial quantum fluctuations, the tensor ones and the scalar ones. Why is r so important? Because, if nothing changes, it is the only new quantity of fundamental physics that we will be able to measure in our epoch.

Every popular model of primordial cosmology has a "prediction" for r. What makes those predictions not reliable is that they are model dependent. High-energy physics, on the other hand, was able to predict the Higgs boson with certainty, through its extremely selective constraints. For an entirely similar reason, it makes the theory of quantum gravity built on the concept of purely virtual particle unique. And so makes a unique, very sharp prediction for r, which lies within less than one order of magnitude:

$$0.0004 < r < 0.0035$$

The present experimental bounds, based on the data collected so far, tell us that r must be smaller than about 0.035. We are not that far away! In a few years (a decade at most) the theory could be spectacularly confirmed. And its confirmation could have the same impact as the discovery of the Higgs boson!

Thus, it may seem that the idea of purely virtual particle does not need particular funding, because its fate is in nature's hands, by now. Yet, I am asking for funding. Why? Because the project I am presenting is not about an idea that can live on itself. This project is about the future of fundamental physics. We just cannot afford another decade like the past ones. The future is in jeopardy here, for generations to come.

The new theory is a "deformation" of the existing theories (standard model + general relativity), so its predictions match the present knowledge in the limit where the purely virtual particle becomes infinitely heavy. But what is crucial is that the theory is able to make new, testable predictions, where nobody else thought it was even possible. And it shows that quantum gravity can be tested well below the Planck scale, precisely in the range of scales of inflationary cosmology.

In the past 5 years I have done a lot of work to study the theory of quantum gravity based on purely virtual particles and the fundamental properties of such particles, with the help of young people and senior colleagues (see the reference list for details). The major outcome of my research activity is the prediction just mentioned. Yet, there is a lot of work to be done, enough to fill a decade or more. The bet is that many more predictions like the one of r are awaiting to be uncovered, and hopefully one of them will trigger the renaissance of particle physics that we desperately need.

As you may easily understand by going through my CV and noting my research independence, which I am extremely proud of, I am not going to wait for Godot anyway, ERC or not ERC. I will be constantly active carrying on the investigation of the fascinating research domain opened up by the idea of purely virtual particle, pursuing all its numerous ramifications, which are: primordial cosmology, collider physics, particle phenomenology, quantum gravity, but also merely theoretical investigations, options to use the new diagrammatics as a mathematical tool to simplify and enhance difficult calculations (see the reference list).

However, the goal of this ERC proposal is not about me or what I have already done. It is about giving the whole of society a chance, turning on a hope in the future for everybody. And injecting it into a dismembered scientific community, which has been wondering around in the dark for decades. The project is about activating a virtuous circle that can hopefully involve everybody in the new endeavour.

You might ask: why hasn't the idea of purely virtual particle already attracted the attention it deserves? The pandemic and the situation that I have described above made it very difficult to get due attention. On top of that, the field is extremely noisy, so every time you say something really new, your voice is covered by a huge background noise. Check again the list of approaches to quantum gravity given above. Any departure from crowded routes is viewed suspiciously. Moreover, simple variations on the mainstream themes are peddled off as out-of-the-box endeavours all the time, causing stress and confusion in evaluators who have a hard time judging proposals and projects. It takes a lot of effort to distinguish a truly out-of-the-box path from the many pretentious ones.

That said, the reactions I have received so far have been quite positive, I have to say. But not "generous" enough in terms of money. Let me briefly summarize them.

Out of 23 papers already published on the theory and its predictions (plus two recent ones, just submitted) in 5 years, 12 have appeared in JHEP, 3 in JCAP, 3 in Phys. Rev. D, 3 in Class. Quant. Grav., 1 in Mod. Phys. Lett. A and 1 in Symmetry. Since the formulation of the theory, I have been invited to give seminars all over the world (27 seminars/talks to conferences in 2.5 years right before the Covid era plus 7 in the Covid era). My main collaborator, M. Piva (once a PhD student under my supervision, now a post-doc) has been invited to give 17 seminars in the pre-Covid era and 8 in the Covid era. The reactions we have received have always been of great interest (typical comments: "I am curious to see where this is going", "will follow developments closely", "if the only price to pay to have a meaningful theory of quantum gravity is renouncing microcausality, we can live with that", etc.).

"Ok, ok" - you may say, - "the reactions have been positive. The idea already has a basis of international recognition. But..."

Let me anticipate your "buts":

- "You know, we have a lot of excellent people to fund. They are waiting in line. You are not the only one."
- "Yes, your ideas are promising, but you do not have enough experience in editorial boards and directions of research groups, and research centres."
- "Ok, a big prediction is backing your claims, but you do not have so many citations. Many people are more cited than you."

Clearly, experience in administrative duties is a must, if you want to solve the problem of quantum gravity. And how can anybody ever solve quantum gravity without being likable, popular and (already) famous?

Let me give you a picture about myself. I have a consistent record of independent research. Just think that 82 of my research papers out of 118 are written by myself as single author. Chasing crowded collaborations may be good to gather citations, but does not increase the chances of making breakthroughs and discoveries. Not only: it has never been my goal to chase crowded collaborations, since I do not see how that can help me solve quantum gravity. Instead, having the highest percentage of single-author papers in the world (in my research domain) is something I am truly proud of. (Also consider that certain health problems have impaired by mobility for 42 months from 2012 to 2016, reducing my research activity and my possibilities to interact with the rest of the community during that period.)

By the way, have you ever thought of normalizing the number of citations to the number of authors of every paper? Do it please, and you will find many surprises. You may even discover that I already have a great impact and a pretty good number of (normalized) citations, larger than the one of many people you think are popular. I let you do this exercise by yourselves, without influencing you more. (And please consider how hard it is to get citations as a single author.)

By choice, I have decided to stay out of editorial boards and avoid bureaucratic work whenever possible. Since recently, I do not even review papers, despite the many invitations I receive from journals. Why? Again: I do not see how that can help me solve quantum gravity. For the same reason, I have never followed mainstreams. I just do not see what good can come from following mainstreams, especially in a period of stagnation like the present one. The possibility to get out of the stalemate can only come from exploring unforeseen, truly high-risk/high-gain new routes. Finally, the research that I have pursued so far, which I also plan to pursue in the future, is hugely demanding. Day after day, hour after hour, it requires a lot of concentration. There is literally no time for much else.

That said, I do have an established track record of leadership and experience in raising young people (about 24 so far - check the next pages). These skills will be crucial for the project, which relies entirely on raising young post docs and students (besides my activity).

Let us now come to the details of the project. I am asking for about 1,800,000 Euros for me, 4 long-term (5, 4 and 3 year long) post doctoral recruitments and one PhD student distributed over the five years of the grant. Equipment costs are negligible with respect to the human costs. I commit to devote 67% of my working time and 100% of my research time to the project.

Let us now plunge into the research goals. The new diagrammatics is rooted in an extremely advanced mathematics, which I cannot describe here. I just list the plan of investigations, leaving the details to part B2. I can distinguish the following main ramifications, each of which can potentially trigger the much needed breakthroughs:

- Primordial cosmology. This is a wide arena for investigations, with the goal to find measurable quantities and suggest new observations or improvements of existing ones. Examples are the deviations from the consistency relation $r + 8n_T \sim 0$, the effects of a nontrivial curvature, the non Gaussianities, the impact of purely virtual particles on the large scale structure of the universe (if the mass of the purely virtual particle of quantum gravity were a bit smaller than it is, we would be living in a scrambled universe).
- Cosmic renormalization-group (RG) flow. The PI has recently shown that primordial inflation can be seen as an RG flow [5], where the perturbation spectra obey an equation of the Callan-Symanzik type, with a very interesting beta function, which can even be studied exactly. This opened a whole new research line [6,7,8], and allows us to enhance the power of our calculations, both with and without purely virtual particles, in view of extracting new testable predictions.
- Particle phenomenology and collider physics. Purely virtual particles allow us to evade several phenomenological constraints that limit the applicability of usual particles [9,10]. This gives us the possibility of building unforeseen models of new physics beyond the standard model, find the answer to

the problem of dark matter, uncover crucial clues for future colliders, propose solutions to existing discrepancies.

- Understanding the ultraviolet limit of quantum gravity. A recent paper [16] by M. Piva, who will collaborate to the project, has recently opened another research line, by showing that it possible to have asymptotic freedom in quantum gravity, if we couple it to certain higher-spin massive multiplets and make use of purely virtual particles. Searching for fixed points of RG flows with purely virtual particles can uncover the secrets of high-energy physics and guide the search of other predictions.
- The new diagrammatics is also a powerful mathematical tool to enhance calculations in gauge theories and the standard model and make it easier to work out testable predictions for future accelerators.

A lot of research experience taught me that the goals need to be refined and updated every year, if not every six months. New research lines appear along the way, as soon as others are completed. This is a bonus, because normally the breakthroughs are never where you look. (Just think that I found the purely virtual particle by searching for the exact opposite...)

Q&A Here are answers to the questions and doubts you might have.

- How can we achieve the renaissance of particle physics, concretely? Not by investing on social gatherings, conferences or public relations, mainstreams or hyper covered paths, but on bold new ideas rooted in the successes of the past, and solid discoveries like the prediction of r mentioned above.
- Can I guarantee that some other, crucial testable prediction will be made during the grant? Of course not (this is the high risk part), but I can guarantee that there are high chances to make the breakthroughs we need. This confidence is based on:
  - having already made one major testable prediction;
  - being the research domain so new that a huge part of it has yet to be explored;
  - being the idea so general that it has ramifications all over the physics of the fundamental interactions.
- Can I guarantee that the scientists around the world will follow through? And that the scientific community will wake up from its lethargy, thereby triggering the much awaited renaissance of particle physics (this is the high-gain part)? Only new experimental data can have the strength to provide the needed shock right away. Before that, it is hard to predict the social tendencies, let alone govern them. What we can achieve is a gradual progression towards a higher and higher probability of making the dream come true, even before the seal of new experimental data. A first help comes from the validation stemming from the approval of this proposal. A second help comes from the larger number of people working on purely virtual particles, thanks to the grant. This can speed up the process, increase the amount of results and the frequency of the publications, driving the necessary attention. But the most important leap forward will come from other testable predictions like the one of r, the breakthroughs mentioned above. They will open the possibility of unifying different groups under the spell of new, ambitious ideas with wide ranges of applications and a common ground.

Summarizing:

1. The project is new and ambitious. Nobody else has been even thinking of something like this.
2. It is solid and doable. It is grounded on a number of results that have already been obtained in the past five years, including the sharp prediction of the tensor-to-scalar ratio r, which could be confirmed within a decade.
3. It is not a continuation of something already going on (which will go on anyway thanks to the stubbornness of the PI...). The goal of the project is to wake up a sleeping scientific community from its lethargy and trigger a long lasting renaissance in particle physics, to revive the spirit of young generations and their faith in the future.
4. The project is necessary, because of the present concerning situation. Important, because without hope in the future science is going to die. Crucial, because society is doomed without a healthy science and a positive view of the future. Timely, because there is no time to waste: the longer the stagnation, the slimmer the chances to get out of it.
5. The project revolves around the PI and his committment to raising new researchers. In this sense, it is not a collaborative project meant to feed a new subcommunity to be added to the long list of existing subcommunities that do not talk to one another. The PI has no interest in “making society” as people usually do. The project is about society as a whole, with an impact that can be far reaching and break the walls that divide the present scientific community.
6. The PI is the only person in the world who can develop these research lines in this very moment. Hopefully, others will replace him in the future. Moreover, the idea of purely virtual particle and its ramifications and applications all over physics is the only idea in the world that has the potential to trigger a complete upside down, yet deeply rooted in the methods and successes of the glowing past.

References

This is the list the papers published so far by the PI and his collaborators on purely virtual particles, the theory of quantum gravity built on this idea, the applications to primordial cosmology, collider physics and other areas. You can retrieve the arXiv papers by clicking on the corresponding links. All the arXiv versions match the final, published journal versions.

THE NEW CONCEPT

The **diagrammatics** of purely virtual particles was developed to the fullest only recently, in the paper

[1] D. Anselmi, *Diagrammar of physical and fake particles and spectral optical theorem*, J. High Energy Phys. 11 (2021) 030 and arXiv :2109.06889 [hep-th].

This paper contains much more powerful and wide ranging results, which apply to all quantum field theories and reduce the key problem of unitarity to a set of merely algebraic operations, advancing our knowledge considerably in this field.

The theory of **quantum gravity** based on purely virtual particles (also called fakeons, or fake particles) was formulated in

[2] D. Anselmi, *On the quantum field theory of the gravitational interactions*, J. High Energy Phys. 06 (2017) 086 and arXiv: 1704.07728 [hep-th].

The concept of fakeon was first introduced in

[3] D. Anselmi, *Fakeons and Lee-Wick models*, J. High Energy Phys. 02 (2018) 141 and arXiv: 1801.00915 [hep-th].

THE IMPLICATIONS ON PRIMORDIAL COSMOLOGY

The first investigations on the implications of the theory on primordial cosmology and the **prediction about the tensor-to-scalar ratio *r*** can be found in

[4] D. Anselmi, E. Bianchi and M. Piva, *Predictions of quantum gravity in inflationary cosmology: effects of the Weyl-squared term*, J. High Energy Phys. 07 (2020) 211 and arXiv:2005.10293 [hep-th].

The **reformulation of primordial inflation as a renormalization-group flow** is in

[5] D. Anselmi, *Cosmic inflation as a renormalization-group flow: the running of power spectra in quantum gravity*, J. Cosmol. Astropart. Phys. 01 (2021) 048 and arXiv: 2007.15023 [hep-th]

The papers

[6] D. Anselmi, *High-order corrections to inflationary perturbation spectra in quantum gravity*, J. Cosmol. Astropart. Phys. 02 (2021) 029 and arXiv: 2010.04739 [hep-th]

[7] D. Anselmi, F. Fruzza and M. Piva, *Renormalization-group techniques for single-field inflation in primordial cosmology and quantum gravity*, Class. Quantum Grav. 38 (2021) 225011 and arXiv: 2103.01653 [hep-th]

[8] D. Anselmi, *Perturbation spectra and renormalization-group techniques in double-field inflation and quantum gravity cosmology*, J. Cosmol. Astropart. Phys. 07 (2021) 037 and arXiv: 2105.05864 [hep-th]

Have developed the analysis of inflation as a cosmic RG flow further, in single-field and double-field inflation.

The cosmological research line needs to be developed in several directions.

FIRST EXPLORATIONS OF THE IMPLICATIONS
ON PARTICLE PHENOMENOLOGY AND COLLIDER PHYSICS

This research line is only at the beginning and has to be explored to the fullest, with enormous potential impact on the new experiments. The papers

[9] D. Anselmi, K. Kannike, C. Marzo, L. Marzola, A. Melis, K. Müürsepp, M. Piva and M. Raidal, *A fake doublet solution to the muon anomalous magnetic moment*, Phys. Rev. D 104 (2021) 035009 and arXiv:2104.03249 [hep-ph]

[10] D. Anselmi, K. Kannike, C. Marzo, L. Marzola, A. Melis, K. Müürsepp, M. Piva and M. Raidal,

Phenomenology of a Fake Inert Doublet Model, J. High Energy Phys. 10 (2021) 132 and arXiv:2104.02071 [hep-ph]

describe examples of applications and strategies for future investigations.

The paper

[11] D. Anselmi, *On the nature of the Higgs boson,* Mod. Phys. Lett. A 34 (2019) 1950123 and arXiv: 1811.02600 [hep-th]

explores the possibility that the Higgs boson is a purely virtual particle, which is not ruled out by data, yet.

The paper

[12] D. Anselmi, *Dressed propagators, fakeon self-energy and peak uncertainty*, arXiv: 2201.00832 [hep-ph]

shows that the width of a purely virtual particle is associated with a new uncertainty principle, due to the fact that it is impossible to "approach the particle too closely", because it "refuses to be brought to reality".

UNIQUENESS AND UNIVERSALITY

Paper [2] shows that the quantum gravity theory based on purely virtual particles is unique. Aside from that, the concept of purely virtual particle is also unique. In the paper

[13] D. Anselmi, *The quest for purely virtual quanta: fakeons versus Feynman-Wheeler particles,* J. High Energy Phys. 03 (2020) 142 and arXiv:2001.01942 [hep-th]

it is shown that the option to define purely virtual particles by following an old approach by Feynman and Wheeler does not work, because its violates fundamental principles.

For different reasons, another alternative, which dates back to Lee and Wick, is unsatisfactory as a fundamental approach (but might work at the effective level), as shown in

[14] D. Anselmi, *Fakeons versus Lee-Wick models: physical Pauli-Villars fields, finite QED and quantum gravity*, arXiv: 2202.10483 [hep-th]

RG FLOW, CONFORMAL FIXED POINTS, THE A-THEOREM with purely virtual particles

In the paper

[15] D. Anselmi, *Quantum field theories of arbitrary-spin massive multiplets and Palatini quantum gravity*, J. High Energy Phys. 07 (2020) 176 and arXiv: 2006.01163 [hep-th]

I have shown that fakeons can be used to make sense of higher-spin massive particles (by embedding them into suitable multiplets). This opens the way to study Palatini quantum gravity and all its implications. Moreover, recently Piva in

[16] M. Piva, *Massive higher-spin multiplets and asymptotic freedom in quantum gravity*, Phys. Rev. D 105 (2022) 045006 and arXiv:2110.09649 [hep-th]

has shown that these multiplets can change the ultraviolet behaviour of quantum gravity, opening the way to the possibility of making it asymptotically free like quantum chromodynamics. These results lead to several new ramifications, from conformal fixed points to reductions of couplings, which could even let us understand the irreversibility of the RG flow (a-theorem).

THE VIOLATION OF MICROCAUSALITY

An important (expected) consequence of purely virtual particles is that causality is violated at very high energies. The first investigations of this property are in

[17] D. Anselmi, *Fakeons, microcausality and the classical limit of quantum gravity,* Class. and Quantum Grav. 36 (2019) 065010 and arXiv: 1809.05037 [hep-th].

[18] D. Anselmi, *Fakeons and the classicization of quantum gravity: the FLRW metric,* J. High Energy Phys. 04 (2019) 61 and arXiv: 1901.09273 [gr-qc]

[19] D. Anselmi and A. Marino, *Fakeons and microcausality: light cones, gravitational waves and the Hubble constant,* Class. And Quantum Grav. 37 (2020) 095003 and arXiv: 1909.12873 [hep-th].

This topic might lead to the identification of unforeseen ways to test the idea experimentally.

OTHER RAMIFICATIONS

A first example of methods to **use purely virtual particles as mathematical tools** was laid out in

[20] D. Anselmi, *Fakeons, unitarity, massive gravitons and the cosmological constant,* J. High Energy Phys. 12 (2019) 027 and arXiv: 1909.04955 [hep-th]

by using Faddeev-Popov fakeons, instead of Faddeev-Popov ghosts. However, this is a path that needs to be pursued along several other directions, because it has a potentially great impact.

The new concept of purely virtual particle was to some extent **inspired by the results of earlier investigations** on some related models, the Lee-Wick models (but note that the Lee-Wick models are not satisfactory, as explained above, and the theory of quantum gravity based on fakeons is not a Lee-Wick model):

[21] D. Anselmi and M. Piva, *Perturbative unitarity of Lee-Wick quantum field theory,* Phys. Rev. D 96 (2017) 045009 and arXiv: 1703.05563 [hep-th]

[22] D. Anselmi and M. Piva, *A new formulation of Lee-Wick quantum field theory,* J. High Energy Phys. 06 (2017) 066 and arXiv: 1703.04584 [hep-th].

The calculations of the **basic properties** of the theory of quantum gravity with fakeons (renormalization, absorptive part of the two-point function) can be found in

[23] D. Anselmi and M. Piva, *Quantum gravity, fakeons and microcausality,* J. High Energy Phys. 11 (2018) 21 and arXiv: 1806.03605 [hep-th].

[24] D. Anselmi and M. Piva, *The ultraviolet behavior of quantum gravity,* J. High Energy Phys. 05 (2018) 27 and arXiv: 1803.07777 [hep-th].

Reviews can be found in

[25] M. Piva, *On the behavior of gravitational force at small scales*, Int. J. Mod. Phys. D 28 (2019) 1944007 and arXiv: 1905.06516 [hep-th]

[26] D. Anselmi, *Purely virtual particles in quantum gravity, inflationary cosmology and collider physics,* Symmetry 2022, 14(3), 521 and arXiv: 2203.02516 [hep-th].

[27] D. Anselmi, *Fakeons, quantum gravity and the correspondence principle,* in "*Progress and Visions in Quantum Theory in View of Gravity: Bridging foundations of physics and mathematics*", F. Finster, D. Giulini, J. Kleiner and J. Tolksdorf editors, Birkhäuser Verlag (2019), arXiv:1911.10343 [hep-th]